Research Policy 37 (2008) 806–822

Contents lists available at ScienceDirect

Research Policy

journal homepage: www.elsevier.com/locate/respol

Two for the price of one? Additionality effects of R&D subsidies: A comparison between Flanders and Germany

Kris Aerts[a,*], Tobias Schmidt[b]

[a] K.U. Leuven, Department of Managerial Economics, Strategy and Innovation (MSI), Naamsestraat 69, P.B. 03535, BE-3000 Leuven, Belgium
[b] Deutsche Bundesbank, Economic Research Centre, Wilhelm-Epstein-Strasse 14, D-60431 Frankfurt am Main, Germany

ARTICLE INFO

Article history:
Received 28 September 2006
Received in revised form 1 October 2007
Accepted 15 January 2008
Available online 16 April 2008

JEL classification:
C14
C21
H50
O38

Keywords:
R&D
Subsidies
Policy evaluation
Conditional difference-in-differences

ABSTRACT

We empirically test whether public R&D subsidies crowd out private R&D investment in Flanders and Germany, using firm level data from the Flemish and German part of the Community Innovation Surveys (CIS III and IV). Both the non-parametric matching estimator and the conditional difference-in-differences estimator with repeated cross-sections (CDiDRCS) clearly indicate that the crowding-out hypothesis can be rejected: funded firms are significantly more R&D active than non-funded firms. In the domain of additionality effects of R&D subsidies, this paper is the first to apply the CDiDRCS method.

1. Introduction

Especially against the background of the knowledge economy, innovation nowadays is deemed to be the main driving force of a country's competitive strength (see e.g. Griliches, 1986). The European Union aspires to become the most competitive economy in the world and proclaims innovation as one of the key pillars in its policy to achieve this (Commission of the European Communities, 2000). In the 2000 Lisbon Strategy an ambitious plan was initiated to leverage the EU's R&D expenditure to 3% of GDP by 2010; of which 2% should be privately financed. However, an intermediate evaluation revealed that instead of rising, the EU R&D expenditure is currently even declining. Recent statistics show that the EU25 spent 1.77% of its GDP on R&D activities in 2004. In the US the R&D expenditure amounted to 2.68% of the GDP and in Japan this number rose to 3.18% (OECD, 2006b). Therefore, the European Commission recently launched an integrated innovation/research action plan, which calls for a major upgrade of the research and innovation conditions in Europe. Mobilizing EU funds and instruments to support research and innovation is one of the objectives formulated in this plan.

Government intervention in the domain of private R&D activities is justified by the argument of market imperfection and is since long time common practice in most industrialized countries. R&D entails the non-excludability characteristic of a public good (see e.g. Samuelson, 1954). Arrow (1962, p. 615) states that "*No amount of legal protection can make a thoroughly appropriable commodity of something as intangible as information. The very use of the information in any productive way is bound to reveal it, at least in part. Mobility of personnel among firms provides a way of spreading information. Legally imposed property rights*

* Corresponding author. Tel.: +32 16 32 68 97; fax: +32 16 32 57 99.
E-mail address: kris.Aerts@econ.kuleuven.be (K. Aerts).

0048-7333/$ – see front matter
doi:10.1016/j.respol.2008.01.011

can provide only a partial barrier, since there are obviously enormous difficulties in defining in any sharp way an item of information and differentiating it from similar sounding items."

Private investments in R&D can never be fully appropriated because other companies have the opportunity to free ride. This leads to underinvestment in R&D activities: the level of R&D expenditure will be below the socially desirable optimum. Public funding reduces the price of socially valuable R&D projects for private investors to a level at which it becomes profitable for companies to invest.

The big challenge for governments obviously is to allocate public funding only to those projects that are socially beneficial and would not be carried out in the absence of a subsidy. This is however not straightforward as companies always have an incentive to apply for public funding. It could be the case that a subsidy merely replaces – or crowds out – private money and does not generate additional R&D investments. The key question in this evaluation problem is: "How much would a firm that has received a subsidy, have spent on R&D if it would not have been subsidized?". Several methods are developed to tackle this question. Examples are the so-called matching estimator and the conditional difference-in-differences (CDiD) method.

This paper provides empirical evidence on the relationship between public R&D funding and private R&D efforts in Flanders and Germany. In a survey of the literature on additionality effects of R&D subsidies, David et al. (2000) conclude that the results of evaluation studies in this field are inconclusive as some report crowding-out effects while others reject them. They attribute this to the fact that researchers use very different databases and econometric methods resulting from differences in information availability in different countries. Therefore it is useful to compare the impact of funding in different countries using similar methods and datasets.

After this brief introduction the reader is guided through the relevant literature. The selection bias and the methodology that we employ to circumvent this problem is explained in the subsequent section. The fourth section entails the description of the specific science and technology (S&T) policy conducted in both countries and the data which will be used. In the fifth section the empirical evidence is presented. The last section contains some concluding remarks.

2. Literature review

Our paper is situated in the domain of input additionality and addresses the issue of crowding-out effects of subsidized R&D. David et al. (2000) conclude in their review of evaluation studies on innovation input that the results on potential crowding-out effects are ambiguous, and they criticize that most existing studies neglect the problem of sample selection bias. That is, R&D intensive firms may well be more likely to apply for a subsidy. Moreover, the government may just as well be more inclined to grant them a subsidy. This makes R&D funding an endogeneous variable, which should be tackled in an adequate way. We will extensively come back to this problem in the next section. Consequently, in more recent research the potential sample selection bias is taken into account through selection models, instrumental variable (IV) estimations (including simultaneous equation systems), difference-in-differences (DiD) estimations and matching techniques.

Although recent studies correcting for a potential selection bias tend to reject full crowding-out effects, the results are ambiguous: Aerts and Czarnitzki (2004, 2006), Almus and Czarnitzki (2003), Czarnitzki (2001), Czarnitzki and Fier (2002), Duguet (2004), Fier (2002), González and Pazó (2006), González et al. (2005), Görg and Strobl (2007), Hussinger (2008) and Lööf and Heshmati (2005) reject full crowding-out effects, while Busom (2000), Heijs and Herrera (2004), Kaiser (2004), Lach (2002), Suetens (2002) and Wallsten (2000) find indications that public R&D funding replaces private R&D investments to some extent. Key reasons for these diverging conclusions are the use of different estimators, as well as the application for a broad range of countries, each with their own specific S&T policy. So far, Denmark, Flanders, France, Germany, Ireland, Israel, Spain, Sweden and the US have been subject to an R&D input evaluation analysis of their public R&D funding system.

All studies on German data reject the full crowding-out hypothesis. Different subsets are analyzed: the service sector (Czarnitzki and Fier, 2002), the manufacturing sector (Fier, 2002; Hussinger, 2008) or more specific East German manufacturing firms (Czarnitzki, 2001; Almus and Czarnitzki, 2003), using nearest neighbor matching approaches (Almus and Czarnitzki, 2003; Czarnitzki, 2001; Czarnitzki and Fier, 2002; Fier, 2002) as well as parametric and semi-parametric two-step selection models (Hussinger, 2008).

The results for Flanders are less clear. Suetens (2002) applies an IV framework on a panel of Flemish firms, but the results are by and large not significant and full crowding-out cannot be rejected. Aerts and Czarnitzki (2004) address the additionality issue with the nearest neighbor matching technique on a cross-section of Flemish manufacturing and selected service companies and extend their research in an IV framework adding information on the amount of subsidy (Aerts and Czarnitzki, 2006). They find evidence that both full and partial crowding-out effects can be rejected.

The work of Görg and Strobl (2007) is of particular relevance for our research. They employ the conditional difference-in-differences technique using a rich panel data set of Irish manufacturing plants. They allow for a certain degree of heterogeneous treatment effects, distinguishing between small, medium and large grants and add the dimension of foreign ownership, given the importance of foreign multinational companies in Ireland. They reject crowding-out of small/medium grants and find additionality effects of small grants. However, they cannot reject crowding-out for foreign plants.

Kaiser (2004) employs a simultaneous probit model and Kernel matching for Denmark and does not find significant proof to reject the crowding-out hypothesis. Duguet (2004) positively evaluates the French R&D subsidy system in a matching framework, with a large panel of manufacturing and service firms. Lach (2002) applies different estimators, such as difference-in-differences and dynamic panel data models and finds large additionality effects in small

Israeli manufacturing firms, but none for large firms. Busom (2000), González et al. (2005), González and Pazó (2006) as well as Heijs and Herrera (2004) analyze Spain. Busom (2000) applies an econometric selection model on a cross-sectional sample of manufacturing firms and concludes that public funding induces more effort for the majority of firms in her sample, but for 30% of the participants, complete crowding-out effects cannot be ruled out. Heijs and Herrera (2004) also analyze a cross-section of manufacturing firms and although they find positive treatment effects, the overall additionality effect is small when the amount of subsidy is taken into account. González et al. (2005) and González and Pazó (2006) investigate subsidies in an unbalanced panel of manufacturing firms, employing nearest neighbor matching and a simultaneous equation model with thresholds. Their analysis rejects full crowding-out effects but does not confirm that public R&D subsidies stimulate private R&D expenditure. Lööf and Heshmati (2005) evaluate the Swedish subsidy policy with nearest neighbor and Kernel matching and reject crowding-out effects. Wallsten (2000) uses a simultaneous equations model and finds that US SBIR grants crowd out private investment dollar for dollar. However, he points out that the program still could have positive effects as the recipient firms might have been able to keep their innovation activities constant while in the absence of a subsidy they might have had to reduce them.

3. Methodology

As the literature overview shows, a range of econometric methods is available to correct for the selection bias. In the following we first expound on this endogeneity problem and then we elaborate on the methods employed here, i.e. the matching estimator and the ordinary and conditional difference-in-differences method.

3.1. Selection bias

We empirically evaluate the effect of public R&D funding. The average impact of a subsidy can be computed as follows:

$$\alpha_{TT} = E(Y_i^T|S_i = 1) - E(Y_i^C|S_i = 1), \tag{1}$$

where Y is the outcome variable (e.g. R&D expenditure) of firm i, in the so-called treated (T) and counterfactual (C) situation, S is the treatment status ($S=1$: treated; $S=0$: untreated-treatment is the receipt of a subsidy in our case). So α_{TT}, the average impact of the treatment on the treated firms, results from comparing the actual outcome of subsidized firms with their potential outcome in case of not receiving a grant. The approach of measuring potential outcomes goes back to Roy (1951). The actual outcome $E(Y_i^T|S_i = 1)$ can be estimated by the sample mean of the outcome in the group of subsidized firms.

The counterfactual situation $E(Y_i^C|S_i = 1)$ can however never be observed and has to be estimated. In a hastily analysis a researcher could compare the average R&D spending of subsidized and non-subsidized companies to compute the treatment effect on the treated, assuming that:

$$E(Y_i^C|S_i = 1) = E(Y_i^C|S_i = 0). \tag{2}$$

However, subsidized companies may well have been more R&D active than the non-subsidized companies even without the subsidy program, which would imply a selection bias in the estimation of the treatment effect. Firms that already are innovative and very R&D active may be more likely to receive an R&D subsidy, as governments want to maximize the probability of success and therefore may well cherry-pick proposals of companies with considerable R&D expertise. Moreover, it is also quite possible that only particular companies apply for public R&D grants because they have an information advantage and are acquainted with policy measures they qualify for. Expression (2) only holds in an experimental setting where there would be no selection bias and subsidies are granted randomly to firms. This is most likely not to be the case in current innovation policy practice.

As the highest expected success is correlated with current R&D spending, the subsidy receipt (treatment) becomes an endogenous variable. To estimate treatment effects while taking this potential endogeneity problem into account, econometric literature has developed a range of methods (see e.g. the surveys of Heckman et al., 1999; Blundell and Costa Dias, 2000, 2002). Examples of these methods are selection models, instrumental variable estimations (including simultaneous equation systems), difference-in-differences estimations and matching. For the application of IV estimators and selection models, valid instruments for the treatment variables are needed. In the case of R&D additionality analysis it is very difficult to find valid instruments, as these should determine the treatment (subsidy receipt) but not the outcome (R&D activities). The difference-in-differences method requires panel data with observations before and after (or while) the treatment. The matching estimator offers the advantage over IV and selection models that no assumptions have to be made, neither on the functional form of the outcome equation nor on the distribution of the error terms of the selection and outcome equation. The disadvantage is that it only allows controlling for observed heterogeneity among treated and untreated firms. To counter this problem and control for unobserved heterogeneity, the conditional difference-in-differences method was developed, which combines the ordinary difference-in-differences estimation with matching. In the following we will expound the matching estimator, the difference-in-differences estimator and the combination of these two.[1]

3.2. Matching estimator

The matching estimator is a non-parametric method and its main advantage is that no particular functional form of equations has to be specified. The disadvantages are strong assumptions and heavy data requirements. The main purpose of the matching estimator is to re-establish

[1] See Aerts et al. (2007) for an explanation of other techniques used in evaluation econometrics.

the conditions of an experiment. The matching estimator attempts to construct a correct sample counterpart for the treated firms' outcomes if they had not been treated by pairing each treated firm i with members h of a comparison group. Under the matching assumption, the only remaining difference between the two groups is the actual subsidy receipt. The difference in outcome variables can then be attributed to the subsidy.

Rubin (1977) proved that the receipt of subsidies and potential outcome are independent for firms with the same set of exogenous characteristics $X = x$:

$$Y_i^{\mathrm{T}}, Y_i^{\mathrm{C}} \perp S_i | X_i = x. \tag{3}$$

This crucial conditional independence assumption (CIA) helps to overcome the problem that the counterfactual outcome $E(Y_i^{\mathrm{C}}|S_i = 1)$ is unobservable. If the CIA holds, the expected outcome $E(Y_i^{\mathrm{C}}|S_i = 0, X_i = x)$ can be used as a measure of the potential outcome of the subsidy recipients. However, the CIA is only fulfilled if all variables X influencing the outcome Y and selection status S are known and available in the dataset. This imposes heavy requirements on the richness of the dataset. If the relevant variables are known and available and the CIA holds, the equation

$$E(Y_i^{\mathrm{C}}|S_i = 1, X_i = x) = E(Y_h^{\mathrm{C}}|S_h = 0, X_h = x) \tag{4}$$

is valid and the average outcome of subsidized firms in the absence of a subsidy can be calculated from a sample of comparable (i.e. matched) firms.

Another feature the matching procedure relies on, is the compliance with the stable unit treatment value assumption (SUTVA), which requires that the potential outcome for each treated firm is stable: it should take one single value (and not follow a distribution) and the treatment of one firm should not affect the treatment effect on another firm (Rubin, 1990). Unfortunately this cannot be tested.

In the matching process for all treated firms i a valid counterpart h should be found in the non-treated population and every firm should represent a potential subsidy recipient. Therefore, we impose a so-called common support restriction. If the samples of treated and non-treated firms would have no or only little overlap in the exogenous characteristics X, matching is not applicable to obtain consistent estimates. If the assumptions hold, the average treatment effect on the treated would consequently amount to

$$\alpha_{\mathrm{TT}}^{\mathrm{M}} = E(Y_i^{\mathrm{T}}|S_i = 1, X_i = x) - E(Y_h^{\mathrm{C}}|S_h = 0, X_h = x) \tag{5}$$

which can be estimated using the sample means of both groups.

In the ideal case, the matching procedure includes as many matching arguments X as possible to find a perfect twin in the control group of non-treated firms for each treated firm. However, the more dimensions that are included, the more difficult it becomes to find a good match: the so-called curse of dimensionality enters. Rosenbaum and Rubin (1983) showed that it is valid to reduce the number of matching dimensions X to a single index: the propensity score $\hat{P}(X)$, which is the probability to receive a subsidy. Lechner (1998) suggested a hybrid matching, where the propensity score $\hat{P}(X)$ and a subset of X condition the matching procedure. This increases the accurateness of the matching procedure, since the equivalence of these extra variables is explicitly imposed, in addition to their value in the propensity score.

Having defined the neighborhood of similar non-treated firms h for each treated firm i, the next issue is the choice of appropriate weights w_{ih} for non-treated observations j within the neighborhood, so that $\alpha_{i,\mathrm{TT}}$ can be computed as

$$\alpha_{i,\mathrm{TT}}^{\mathrm{M}} = Y_i^{\mathrm{T}} - \sum_{h=1}^{N} w_{ih} Y_h^{\mathrm{C}}. \tag{6}$$

Two commonly used procedures are Kernel-based matching and nearest neighbor. In the Kernel-based matching, a treated firm is matched to all non-treated firms in the control group, but the controls are weighted according to the Mahalanobis distance between the treated firm and each non-treated firm. We will employ nearest neighbor matching. This technique matches a treated firm i to the non-treated firm h in the control group that is closest in terms of the Mahalanobis distance between the respective propensity scores and possible other matching arguments. The nearest neighbor can be selected with or without replacement. To obtain the best possible match a large pool of controls is required. Therefore, we employ matching with replacement and allow different treated firms to be matched to the same non-treated firm. This will cause a bias in the ordinary t-statistic on mean differences which has to be corrected for (Lechner, 2001). The detailed matching protocol is depicted in Table 1.

3.3. Difference-in-differences (DiD) estimator

In the difference-in-differences model the estimation of the treatment effect is based on the idea that the counterfactual outcome of a subsidized firm i in period t_1 can be approximated by the outcome of that treated firm in an earlier period t_0 where it did not receive a subsidy. To control for macroeconomic changes over time DiD relates the development of subsidized firms i to a control group of non-subsidized firms h and compares them before (t_0) and after (t_1) the treatment moment:

$$\begin{aligned}\alpha_{\mathrm{TT}}^{\mathrm{DiD}} &= (Y_{i,t_1}^{\mathrm{T}} - Y_{i,t_0}^{\mathrm{C}} | X_{i,t_0}, X_{i,t_1}, S_{i,t_0} = 0, S_{i,t_1} = 1)\\ &\quad - (Y_{h,t_1}^{\mathrm{C}} - Y_{h,t_0}^{\mathrm{C}} | X_{h,t_0}, X_{h,t_1}, S_{h,t_0} = 0, S_{h,t_1} = 0)\end{aligned} \tag{7}$$

Fig. 1 depicts the DiD methodology. Evolutions B and C are evaluated over time. The DiD technique allows controlling for both common macroeconomic trends and constant individual-specific unobserved effects. Besides the outcome and treatment variables, additional covariates X enter Eq. (7) to account for the possibility that the treated and non-treated samples have systematically different characteristics in t_0 and t_1 (see Wooldridge, 2002). Neither functional form nor regressor is required for the outcome measure. However, a big disadvantage is that panel data are necessary, including observations before and after (or while) the treatment. As subsidies often target longer term research projects, and firms may receive multiple grants over time, it is difficult to construct a database that is suited

Table 1
Matching protocol (nearest neighbor matching)

Step 1	Specify and estimate a probit model to obtain the propensity scores $\hat{P}(X)$.
Step 2	Restrict the sample to common support: delete all observations on treated firms with probabilities larger than the maximum and smaller than the minimum in the potential control group. (This step is also performed for other covariates that are possibly used in addition to the propensity score as matching arguments.)
Step 3	Choose one observation from the subsample of treated firms and delete it from that pool.
Step 4	Calculate the Mahalanobis distance MD between this firm and all non-subsidized firms in order to find the most similar control observation. $\mathrm{MD}_{ih} = (Z_h - Z_i)'\Omega^{-1}(Z_h - Z_i)$ In the Flemish case, Z contains the estimated propensity score and the firm size (lnEMP) as additional arguments in the matching function. In the German case, also the dummy that indicates location in East Germany is an additional argument. Ω is the empirical covariance matrix of these arguments, based on the sample of potential controls.
Step 5	Select the observation with the minimum distance from the remaining sample. (Do not remove the selected control from the pool of potential controls, so that it can be used again.)
Step 6	Repeat steps 3–5 for all observations on subsidized firms.
Step 7	Using the matched comparison group, the average treatment effect on the treated can thus be calculated as the mean difference of the matched samples: $\hat{\alpha}^{\mathrm{M}}_{\mathrm{TT}} = \frac{1}{n^{\mathrm{T}}}\left(\sum_i Y_i^{\mathrm{T}} - \sum_i \hat{Y}_i^{\mathrm{C}}\right)$ with $\hat{Y}_i^{\mathrm{C}}$ being the counterfactual for firm i and n^{T} is the sample size (of treated firms). Note that the same observation may appear more than once in that group.
Step 8	As we perform sampling with replacement to estimate the counterfactual situation, an ordinary t-statistic on mean differences is biased, because it does not take the appearance of repeated observations into account. Therefore, we have to correct the standard errors in order to draw conclusions on statistical inference. We follow Lechner (2001) and calculate his estimator for an asymptotic approximation of the standard errors.

for an appropriate application of DiD. Another shortcoming of DiD is that strategic behavior of firms to enter the subsidy program would lead to biased estimates. Moreover, if the companies that do and the companies that do not receive subsidies react differently on macroeconomic changes, the estimates are biased.

3.4. Conditional difference-in-differences (CDiD) estimator

The CDiD estimator combines the advantages of matching and DiD and eliminates some of their respective disadvantages. DiD controls for unobserved heterogeneity between treated and non-treated companies and the matching technique controls for potentially different reactions to macroeconomic changes in the treated and the non-treated group. Heckman et al. (1998) show that CDiD based on a non-parametric matching provides an effective tool in controlling for selection on both observables and unobservables.

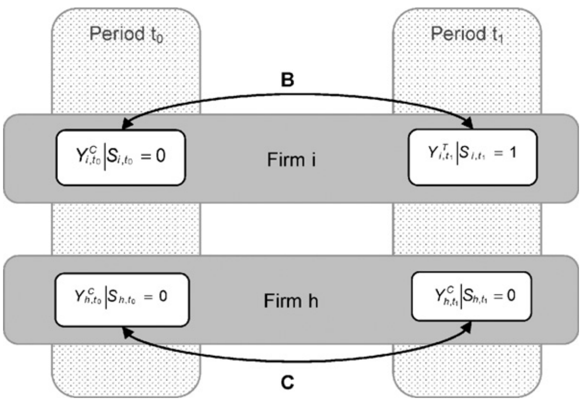

Fig. 1. DiD methodology.

The control group used in the CDiD model is not general as in the ordinary DiD, but is a sample of non-treated firms h which is matched to the treated firms i in the period (t_0) before receiving the treatment (in period t_1). The effect of the treatment on the treated is estimated from the evolution of the two comparable groups over time. Blundell and Costa Dias (2000) suggest employing CDiD[2] with repeated cross-sections (CDiDRCS) if panel data are not available. However, the estimation of the treatment effect may be inconsistent if repeated cross-sectional data are used in a situation where the composition of the groups of treated and non-treated firms changes over time (due to some unknown and unobservable rule) and is affected by the treatment. In this case, the company-specific effect is no longer constant over time, causing a bias in the estimation. This bias adds to the potential residual problem of unobserved effects which is induced even when panel data are used (see Görg and Strobl, 2007). This imposes extra constraints on the data that can be employed. Nevertheless, Blundell and Costa Dias (2000, p. 437) indicate that "*there is a clear trade-off between the available information and the restrictions needed to guarantee a reliable estimator*." As there were no significant changes in the S&T policy between the years under investigation and we have a relatively rich dataset at our disposal, we feel confident in applying the CDiDRCS methodology here. As we will point out later, additional robustness checks support our audacity.

In the CDiDRCS three matchings are required, as depicted in Fig. 2. For every treated firm i in period t_1, a non-treated twin firm h has to be found in the same period t_1 (matching A). In the next step, a control group has to be compiled: for each treated firm i and each non-treated firm h in period t_1 a twin firm, i.e. k and j, respectively, has to be found in period t_0 (matchings B and C). The average treat-

[2] In Blundell and Costa Dias (2000), CDiD is referred to as MMDiD: method of matching with difference-in-differences.

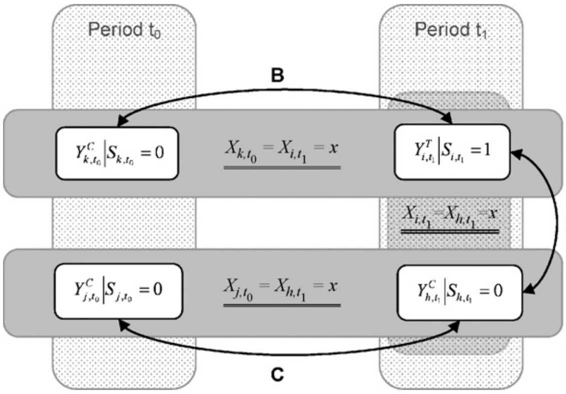

Fig. 2. CDiDRCS methodology.

ment effect on the treated firms then can be estimated as follows:

$$\begin{aligned}\alpha_{\mathrm{TT}}^{\mathrm{CDiDRCS}} = &\,(E(Y^{\mathrm{T}}_{i,t_1}|X_{i,t_1}=x, S_{i,t_1}=1)\\ &- E(Y^{\mathrm{C}}_{k,t_0}|X_{k,t_0}=x, S_{k,t_0}=0))\\ &- (E(Y^{\mathrm{C}}_{h,t_1}|X_{h,t_1}=x, S_{h,t_1}=0)\\ &- E(Y^{\mathrm{C}}_{j,t_0}|X_{j,t_0}=x, S_{j,t_0}=0)) \qquad (8)\end{aligned}$$

4. Empirical analysis: the data

Before we come to the data which are employed in the empirical part of this article, we first go into the details of the public funding system of R&D activities in Flanders and Germany.

4.1. Public funding of R&D in Flanders and Germany

Germany conducts its science and technology policy at the national level while policy makers in Flanders, the largest region in Belgium, operate at the regional level. However, a comparison between Germany and Flanders seems to be a reasonable choice. First, the Belgian S&T policy is highly regionalized: the Flemish public R&D funding falls entirely under the responsibility of the Flemish government and should therefore also be evaluated at the regional Flemish level.

Moreover, the Flemish and German R&D funding systems do not differ substantially. The German public R&D funding relies largely on direct funding of R&D projects of firms and on institutional funding of more basic research. The main federal government agencies providing public funding are the Federal Ministry of Education and Research (BMBF) and the Federal Ministry of Economics and Labor (BMWA). German firms and research institutions also qualify for European funding programs, of course. Fiscal measures, like R&D tax credits, do not exist in Germany.

In Flanders, IWT, the Institute for the Promotion of Innovation through Science and Technology in Flanders, is the single counter where companies can apply for a subsidy. This implies that subsidies, at the Flemish, Belgian and European level, are evaluated and granted through IWT. Accelerated depreciation for R&D capital assets and R&D tax allowances are available through the federal Belgian government. In contrast to most countries, the Belgian R&D tax allowances are fixed and not granted as a percentage: for each additional employee employed in scientific research, the company is granted a tax exemption for a fixed amount, in the year of recruitment. However, as Van Pottelsberghe et al. (2003) indicate, very few Belgian companies actually make use of these fiscal measures. Main reasons are a low level of acquaintance with the system, high administration costs and the fact that the measures are not significantly substantial: e.g. the tax exemption is a short term measure while R&D is typically a long term process. Direct R&D funding through IWT remains the largest source of public R&D grants in the private sector in Flanders.

Germany is a large economy, while Flanders is a small economy, which may induce different impacts of R&D funding. However, even if the additionality effect can be heterogeneous in country size, the comparison remains a useful exercise, as it tends to validate the estimation methods.

4.2. Data

The potential crowding-out effect of R&D subsidies is addressed empirically with data from the Flemish and German[3] Community Innovation Survey (CIS). First, a cross-sectional dataset, i.e. the CIS IV wave covering the years 2002–2004, is used. In a second step, data from the CIS III wave, referring to the years 1998–2000, is additionally plugged into the analysis. The CIS covers most EU countries using a largely harmonized questionnaire in all countries.[4] Our sample covers the Flemish and German manufacturing sector and computer services, R&D services as well as business related services. In accordance with the OECD/Eurostat (1997) guidelines for the CIS survey, the sample is restricted to companies with 10 or more employees. The total sample consists of 4566 (1665) German (Flemish) observations on 3903 (1471) companies: the overlap between the two waves is very limited: only 663 (194) German (Flemish) firms are observed twice. These innovation data are supplemented with patent application data of German and Flemish firms from the European Patent Office, covering all applications from 1978 to 2004.

The receipt of subsidies is denoted by a dummy variable (FUN) indicating whether the firm, observed in the CIS IV (III)[5], received public R&D funding in the period 2002–2004 (1998–2000). On average 20% of the Flemish innovative companies received public funding between 2002 and 2004. The Flemish government provided 68% of these firms with R&D funds; the national and European

[3] Note that the German Community Innovation Survey data is part of the Mannheim Innovation Panel, the annual German innovation survey.

[4] Eurostat (2004) presents detailed descriptive survey results for all countries and aggregate statistics.

[5] In the description of the variables, we always refer to 2 years, i.e. the year of the CIS-wave. For the ordinary matching approach, only the CIS IV is used. In the CDiDRCS approach, the CIS III wave adds a time dimension (2 years earlier).

governments were less, but nevertheless important sources of public R&D funding of Flemish companies (40% and 19%, respectively). At the start of IWT in 1992, the number of approved projects was 35: 28 companies received a total of 13 million EUR. In 2004 IWT approved 425 projects and supported 335 companies with 76 million EUR. The average subsidized amount per company evolved from 0.466 million EUR in 1992, to 0.339 million EUR in 2000 to 0.227 million EUR in 2004. The average number of projects per company slightly oscillates around 1.2.

In 2004 14% of German enterprises with innovative activities received public funding. Fifty five percent of these companies were funded by local or regional authorities. The national government financially supported 54% of these innovative companies and the EU government provided 29% of them with financial support for R&D activities. In the 1980s each year German companies received in total about 394 million EUR for 1623 projects. The number of submitted and granted R&D projects increased steadily over the years, but the average grant size declined. In 2004, 4080 projects were funded, with a total value of 363 million EUR (OECD, 2006a).

We did not distinguish between the different funding sources; the funding impact is an average effect over the different funding schemes. We would also like to stress that the restriction to a dummy variable (instead of using full information on the amount of the subsidy) imposes a limitation on the interpretation of the results. We can only analyze whether there is full crowding-out, i.e. the subsidy fully replaces private money. In this case the actual and counterfactual R&D spending of funded firms would be equal. Partial crowding-out would mean that the subsidy partially replaces private money: the funded companies spend more on R&D, but the additional amount of R&D spending is smaller than the amount of the subsidy. In the case of full additionality, funded companies spend their budgeted R&D expenditure and all additional public money or even more (the subsidy might help the company to bridge some threshold value and enable it to set up a larger R&D project than initially possible). Neither the hypotheses of partial crowding-out and full additionality nor potentially heterogeneous size effects can be tested in the framework presented in this paper. Moreover, the dummy variable implies a drawback on the comparability between the two countries: the effect of the subsidy may be heterogeneous in country size.

As the subsidy dummy covers a 3-year period, we use, whenever possible, values of the covariates measured at the beginning of the reference period, 2002 (1998) in order to avoid endogeneity problems in the selection equation.

We test the hypothesis of input additionality on two outcome variables. First, R&D expenditure[6] at the firm level in 2004 (2000), RD, is evaluated. However, as the distribution of this indicator is very skewed in the economy, we also investigate the R&D intensity, RDINT (R&D expenditure/turnover × 100). Also due to the skewness of RD and RDINT, some extreme values might affect the mean of the distribution significantly, so that a few observations may determine the estimation results. Using the logarithmic transformation scales down the large values and reduces the problem with these skewed distributions. Therefore, the logs[7] of RD and RDINT are additionally evaluated as outcome variables. All outcome variables refer to the year 2004 (2000).

We use several control variables in our analysis which may affect both the probability to receive subsidies and R&D expenditure, respectively. Including the number of employees at the beginning of the period allows controlling for size effects, which are empirically often found to explain innovativeness (see e.g. Veugelers and Cassiman, 1999). Moreover, both the Flemish and German S&T policy put high value on R&D activities performed by small and medium sized companies. Therefore, the size variable is also expected to influence the subsidy receipt. Again, the logarithmic transformation (lnEMP) is used to avoid potential estimation biases caused by skewness of the data.

Another important variable in our analysis is the firms' patent stock (PATST). As we use data from two cross-sectional datasets which do not include time-series information, the patent stock enables us to control for previous (successful) R&D activities. Obviously, not all innovation efforts lead to patents, which Griliches (1990, p. 1669) formulated nicely as "*not all inventions are patentable, not all inventions are patented*". Likewise, not all patented innovations result from R&D activities; the R&D process is only part of a company's innovative activity, which includes, according to the Oslo Manual, the international handbook for conducting innovation surveys worldwide (OECD/Eurostat, 1997, p. 10), "*all those scientific, technological, organisational, financial and commercial steps which actually, or are intended to, lead to the implementation of technologically new or improved products or processes*". Moreover, the propensity to patent may be heterogeneous among firms. However, as data on previous R&D expenditure are not available, the patent stock is the best approximation of past innovation activities we have at our disposal. We use all patent information in the EPO database and generate the stock of patents for each firm i as the depreciated sum of all patents filed at the EPO from 1978 until 2001 (1997):

$$\mathrm{PATST}_{i,t} = (1-\delta)\mathrm{PATST}_{i,t-1} + \mathrm{PATA}_{i,t}, \tag{9}$$

where PATST is the patent stock of firm i in period t and $t-1$, respectively, $\mathrm{PATA}_{i,t}$ are the number of patent applications filed at the EPO and δ is a constant depreciation rate of knowledge which is set to 0.15 as common in the literature (see e.g. Jaffe, 1986; Griliches and Mairesse, 1984). On the one hand, firms that exhibit previous successful innovation projects indicated by patents, are more likely to receive public R&D funding, because the public authorities may follow the 'picking-the-winner' principle in order to minimize the expected failure rates of the innovation projects, and hence, to maximize the expected benefit for the society. On the other hand, the patent stock controls

[6] In the CIS survey, R&D expenditure is defined in accordance with the Frascati Manual (OECD, 1993).

[7] We replaced zero values of RD and RDINT with the minimum observed value, in order to generate the log of the variables.

for the past average innovative engagement of the firms, because it is expected that firms that were highly innovative in the past will continue this strategy. The patents are counted only until 2001 (1997), to ensure that the stock definitely refers to past innovation activities, in order to avoid a simultaneous equation bias in the regression analysis. The patent stock enters into the regression as patent stock per employee to reduce the potential multicollinearity with firm size.

A dummy variable indicating whether a firm belongs to a group (GROUP) controls for different governance structures. Firms that belong to a group may be more likely to receive subsidies because they presumably have better access to information about governmental actions due to their network linkages. In contrast, if firms belong to a group with a foreign parent (FOREIGN), it may be the case that the group tends to rather file subsidy applications in its home country.

The export quota (EXQU = exports/turnover) measures the degree of international competition a firm faces. Firms that engage in foreign markets may be more innovative than others and, hence, would be more likely to apply for subsidies. In the German analysis we also include the variable EAST, indicating whether the firm is located in East Germany. There are strong indications that the innovation behavior of East and West German firms may still be different (see e.g. Aschhoff et al., 2006; Sofka and Schmidt, 2004). Furthermore, special policy programs apply for East German firms, which is obviously important in the framework of additionality effects of R&D subsidies. Typically, companies in East Germany are younger and smaller. Again, we have to point out that the potential 'mismatch' between innovation and R&D activities induces that EXQU and EAST are an indication of a firm's R&D activity.

Finally, some industry dummies (BR) are included to allow for differences between different sectors in the economy. The relationship between size and R&D activities is often found to depend on industry characteristics. Acs and Audretsch (1987), amongst others, conclude that large firms are more innovative when they operate in capital-intensive and highly concentrated sectors, while smaller firms expose a higher degree of innovative activity in industries which are highly innovative and dependent on skilled labor. Moreover, some funding schemes are directly targeted at specific industries or groups of industries, like Biotech programs. Therefore, interaction terms between the industry dummies and lnEMP (BR_lnEMP) are included as well.

All variables described until now, are available in both the German and Flemish dataset (except for the EAST variable which is obviously only included for the German data) to allow a certain degree of comparison of the results. However, as was stressed in the methodological part of this paper, the matching procedure crucially relies on the fulfillment of the conditional independence assumption. Only in that case, the average outcome of subsidized firms in the absence of a subsidy can be estimated based on a sample of comparable-matched-firms. It is arguable that relevant values are missing in the analysis. Therefore, we add some variables in additional robustness analyses, which are however, not available or perfectly comparable for both datasets. Four Flemish and three German variables are generated separately, in addition to the comparable variables described above. PROJ_PAST5YR is a count variable, reflecting the total number of project proposals each Flemish company submitted in order to obtain an R&D subsidy in the proceeding 5 years. It is obtained by merging the firm level CIS/patent information with the project level ICAROS database, in which IWT keeps track of all subsidy applications by Flemish companies. This is a very important control variable (unfortunately only available in the Flemish dataset) since it is very likely highly correlated with both the probability to receive a subsidy and the outcome variable. Companies which submitted many projects in the past are on the one hand more experienced in applying for a subsidy and therefore possibly more 'eligible' for a grant. On the other hand, they may be more innovative and therefore more likely to apply for a subsidy to support their extensive R&D activities. Next, variables reflecting the technological and financial quality of the company might be important. In the Flemish dataset, these characteristics are proxied by capital intensity (CAPINT) as the value of fixed assets and cash-flow (CASHF) (both in million EUR), respectively. Both variables are obtained from balance sheet records provided by the National Bank of Belgium (through the Belfirst database) and divided by the number of employees to avoid multicollinearity with firm size. In the German dataset, information on factors hampering the innovative activity was used to construct measures of a company's technological and financial profile. TECHCONSTR is a four-point-Likert-scale variable (0: not relevant to 3: very important) indicating whether a lack of technological information hindered the company in its innovative activities; FINCONSTR is a four-point-Likert-scale reflecting whether the firm faced financial difficulties in its innovative activities, both internally (innovation activities were too expensive) and externally (difficulties to find external financing of the innovative activities). Finally, we were able to construct a variable SCOMNACE for both datasets, signaling to which extent information from competitors is absorbed by the company. To avoid potential endogeneity with the outcome variables, this variable was rescaled on the three digit (NACE3) industry level.

5. Estimations

We test the additionality hypothesis with two techniques. First, we employ the matching estimator, as common in the literature on the evaluation of R&D subsidies. In the second step, we control for unobserved heterogeneity effects by using the CDiDRCS estimator. This is new in the domain of R&D additionality research. A third section provides additional robustness checks to validate our results.

5.1. The matching estimator

In this subsection the matching estimator is applied to the data of the CIS IV cross-section to estimate the additionality effect of subsidies that were granted to Flemish and German companies between 2002 and 2004. Table 2 presents the descriptive statistics for the samples, which

Table 2
Descriptive statistics of the Flemish and German sample

Variable	Subsidized companies		Potential control group of non-subsidized companies		p-values of two-sided t-tests on mean equality
	Mean	Standard deviation	Mean	Standard deviation	
Flemish sample					
lnEMP	4.198	1.630	3.645	1.273	$p = 0.0000$
PATST	0.800	2.592	0.043	0.325	$p = 0.0002$
GROUP	0.602	0.491	0.449	0.498	$p = 0.0003$
FOREIGN	0.263	0.442	0.222	0.416	$p = 0.2685$
EXQU	0.026	0.092	0.018	0.086	$p = 0.2916$
$\hat{P}(X)$	0.336	0.241	0.159	0.120	$p = 0.0000$
RD	2.002	4.972	0.228	1.166	$p = 0.0000$
RDINT	8.046	14.425	1.096	3.783	$p = 0.0000$
lnRD	−1.200	3.513	−7.213	3.694	$p = 0.0000$
lnRDINT	0.175	2.762	−3.855	2.806	$p = 0.0000$
Number of observations	171		712		
German sample					
lnEMP	4.443	1.679	4.206	1.468	$p = 0.0041$
PATST	0.806	2.127	0.298	1.245	$p = 0.0000$
GROUP	0.660	0.474	0.569	0.495	$p = 0.0002$
FOREIGN	0.127	0.334	0.094	0.291	$p = 0.0393$
EXQU	0.303	0.271	0.166	0.232	$p = 0.0000$
EAST	0.491	0.500	0.280	0.449	$p = 0.0000$
$\hat{P}(X)$	0.351	0.190	0.173	0.145	$p = 0.0000$
RD	8.062	62.051	1.135	6.756	$p = 0.0127$
RDINT	7.227	6.710	1.217	3.445	$p = 0.0000$
lnRD	−0.937	2.697	−4.521	3.054	$p = 0.0000$
lnRDINT	0.376	2.914	−4.278	3.694	$p = 0.0000$
Number of observations	503		1871		

Note: the industry dummies BR and interaction terms BR_lnEMP are not reported here.

consist of 2374 (883) German (Flemish) companies, of which 503 (171) received public funding.

The two-sided t-tests indicate significant differences between the subsidized companies and the potential control group of non-subsidized companies. Flemish and German subsidized firms are larger, have a larger patent stock and are more likely to belong to a group. The dummies for foreign ownership and the export quota do not differ significantly between the Flemish groups. German subsidized firms are more likely to be foreign and have a significantly higher export quota. As expected, also the dummy for companies located in East Germany differs between the two groups. The industry dummies BR and interaction terms BR_lnEMP (not presented in Table 2) are significantly different both in the Flemish and German sample. The outcome variables show that the subsidized companies are significantly more R&D active. However, we cannot simply assign this difference to the subsidy receipt, due to the potential selection bias, which we already described before. Therefore, we have to select a control group that has similar characteristics compared to the group of funded companies.

This control group is selected in accordance with the matching procedure which was outlined in the methodological section of this paper. The first step consists of estimating a probit model on the receipt of subsidies. The estimation results for the Flemish and German sample in Table 3 show that the most important variables are – as expected – size, the patent stock, the group and foreign dummy, the export quota and the East Germany dummy. Further tests show that the interaction terms BR_lnEMP are jointly significant ($\chi^2(11) = 31.51$ and $p = 0.0009$ for the German and $\chi^2(11) = 36.50$ and $p = 0.0001$ for the Flemish sample). As a result, these interaction terms are also included in the propensity score (this probit model is not presented in the paper). In the second step, for each subsidized firm i a twin firm h is selected from the control group of non-subsidized companies with the hybrid nearest neighbor matching technique. In both the Flemish and German S&T policy, size is an important determinant of the probability to receive a subsidy (e.g. given the subsidy programs especially designed for small and medium sized enterprises). Therefore it is explicitly taken into account, next to its implicit value in the propensity score. As mentioned before, this increases the accurateness of the matching. For the matching in the German sample, the dummy indicating whether the company is located in East Germany is included as an additional explicit matching variable. Due to the common support[8] requirement 4 (4) German (Flemish) non-funded firms and 25 (20) funded observations had to be deleted from the sample (CIS III and

[8] As this matching procedure within the CIS IV is the starting point for the CDiDRCS in Section 5.2 where matches to the CIS III are added for the treated and selected non-treated firms from this Section 5.1, we impose the simultaneous common support requirement for all three matchings already in this first step.

Table 3
Probit estimations and marginal effects

	Flemish sample				German sample			
	Probit estimates		Marginal effects		Probit estimates		Marginal effects	
	Coefficient	Standard error	dy/dx	Standard error	Coefficient	Standard error	dy/dx	Standard error
lnEMP	0.168***	0.046	0.042***	0.011	0.048*	0.025	0.012*	0.006
PATST	0.373***	0.101	0.092***	0.025	0.061***	0.210	0.015***	0.005
GROUP°	0.089	0.134	0.022	0.033	0.106	0.072	0.027	0.018
FOREIGN°	−0.300**	0.151	−0.068**	0.031	−0.130	0.107	−0.031	0.024
EXQU	-0.141	0.623	−0.035	0.154	1.091***	0.150	0.275***	0.038
EAST°					0.787***	0.070	0.223***	0.021
Constant	−1.844***	0.187			−1.954***	0.130		
BR	$\chi^2(11) = 26.66$ $p = 0.0052$				$\chi^2(11) = 103.19$ $p = 0.0000$			
Log-likelihood	-379				-1019			
Pseudo R^2	0.076				0.147			
Number of observations	866				2348			

*** (**, *) indicate a significance level of 1% (5%, 10%). The marginal effects on subsidies are calculated at the sample means for continuous variables and for a discrete change of dummy variables (indicated by °) from 0 to 1. Their standard errors are obtained by the delta method. The propensity score used in the matching algorithm takes the interaction terms between size and industry additionally into account. The coefficients change only marginally and are not reported in this paper.

IV together). The likelihood to receive public funding (the propensity score, obtained from the probit model), firm size and for the German sample also the East Germany dummy, are used as arguments in the matching procedure. Table 2 shows that the propensity score is significantly different too between the group of subsidized companies and the potential control group for both samples.

When we only take the selected control group into account in the t-tests (see Table 4) we no longer observe significant differences in the control variables size, patent

Table 4
Descriptive statistics of the Flemish and German matched samples

Variable	Subsidized companies		Selected control group of non-subsidized companies		p-values of two-sided t-tests on mean equality[a]
	Mean	Standard deviation	Mean	Standard deviation	
Flemish sample					
lnEMP	4.129	1.517	4.121	1.493	$p = 0.969$
PATST	0.228	0.788	0.135	0.577	$p = 0.283$
GROUP	0.573	0.496	0.567	0.497	$p = 0.921$
FOREIGN	0.248	0.433	0.197	0.399	$p = 0.340$
EXQU	0.024	0.087	0.015	0.064	$p = 0.396$
$\hat{P}(X)$	0.289	0.175	0.285	0.170	$p = 0.864$
RD	1.287	3.070	0.450	1.184	$p = 0.002$
RDINT	7.240	13.415	2.534	6.278	$p = 0.000$
lnRD	−2.283	3.484	−5.211	4.243	$p = 0.000$
lnRDINT	−0.007	2.792	−2.341	3.265	$p = 0.000$
Number of observations	157		157		
German sample					
lnEMP	4.453	1.647	4.451	1.609	$p = 0.985$
PATST	0.695	1.777	0.522	1.548	$p = 0.164$
GROUP	0.659	0.475	0.688	0.464	$p = 0.418$
FOREIGN	0.126	0.332	0.145	0.352	$p = 0.480$
EXQU	0.291	0.263	0.302	0.300	$p = 0.626$
EAST	0.486	0.500	0.486	0.500	$p = 1.000$
$\hat{P}(X)$	0.338	0.177	0.335	0.174	$p = 0.834$
RD	4.982	20.587	1.750	7.744	$p = 0.002$
RDINT	7.033	9.662	1.707	4.002	$p = 0.000$
lnRD	−0.987	2.686	−3.667	3.457	$p = 0.000$
lnRDINT	0.312	2.942	−3.486	3.899	$p = 0.000$
Number of observations	484		484		

Note: the industry dummies BR and interaction terms BR_lnEMP are not reported here.

[a] t-statistics to test the mean equality between the sample of funded firms and the selected control group are based on Lechner's (2001) asymptotic approximation of the standard errors that accounts for sampling with replacement in the selected control group.

Table 5
Average treatment effects on the treated companies

	Flanders				Germany			
	Absolute		Relative (%)		Absolute		Relative (%)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
RD (in mio EUR)	0.837	0.211	65	89	3.232	0.401	65	100
RDINT (in %)	4.669	1.484	64	91	5.327	3.219	76	100

Table 6
Treatment effect estimations in the three matchings (difference in group means)

	A	B	C
Flemish sample			
RD	0.837 (0.273)***	0.900 (0.288)***	0.050 (0.178)
RDINT	4.669 (1.246)***	5.017 (1.429)***	0.203 (1.190)
lnRD	2.923 (0.512)***	2.530 (0.832)***	−0.480 (0.854)
lnRDINT	2.334 (0.400)***	2.065 (0.635)***	−0.242 (0.646)
German sample			
RD	3.232 (1.049)***	2.432 (1.433)*	−0.262 (2.027)
RDINT	5.327 (0.503)***	5.717 (0.544)***	0.201 (0.939)
lnRD	2.680 (0.245)***	2.956 (0.344)***	0.165 (0.823)
lnRDINT	3.798 (0.274)***	4.052 (0.386)***	0.125 (0.935)

*** (**, *) indicate a significance level of 1% (5%, 10%).
The standard errors (between brackets) are heteroscedastic consistent and the *t*-statistics are based on Lechner's (2001) asymptotic approximation of the standard errors that accounts for sampling with replacement in the selected control group.

stock, group, foreign ownership, export quota, location in East Germany, industry dummies and the propensity score. However, the differences in the outcome variables remain significant: the funded companies are more R&D active; they spend more on R&D both in absolute terms and proportionally to the turnover. We can conclude that for both the Flemish and German sample the crowding-out hypothesis can be rejected: the average R&D expenditure and the average R&D intensity have increased due to the public funding of R&D.

The average treatment effects can be calculated from the sample means in Table 4 and are presented in Table 5. The absolute difference in RD in million EUR and RDINT in % is converted into a relative difference, based on the values for RD and RDINT of the treated group. Strictly speaking, the treatment effect which is calculated in the matching procedure can only be evaluated at the averages of the samples (see Eq. (5)). However, as the distribution of both R&D expenditure and intensity is very skewed, we also calculated the median differences. These results should be interpreted with caution, though. On average, a Flemish company that receives a subsidy, spends 0.837 million EUR (65%) more on R&D, compared to the situation where it would not have received the subsidy. The German subsidized firms spend, on average, 3.232 million EUR (65%) more. The R&D intensity in absolute terms increases with about 5% in Flanders and Germany due to the subsidy. It would be interesting to test the presence of heterogeneous treatment effects: large subsidies could induce other effects than small subsidies. Unfortunately, the available data do not allow us to further investigate this issue.

5.2. The CDiDRCS estimator

The matching estimator indicates that crowding-out effects can be rejected in the Flemish and German case. However, one critique to the matching approach is that it only controls for observed heterogeneity between the subsidized and non-subsidized companies. Therefore, we apply the CDiDRCS estimator, which combines matching with the DiD approach for a set of pooled cross-sectional data. The starting point is the matching result of Section 5.1 (A in Fig. 2). In the CDiDRCS approach, two additional matchings (B and C in Fig. 2) are conducted. For the treated (i) and selected non-treated (h) firms, twin firms (k and j, respectively) are selected from the firms observed in the CIS III. The treatment effect is then calculated from the mean difference between the treated and non-treated firms over time. In this way, both unobserved heterogeneity and potentially different reactions to macroeconomic changes in the treated and the non-treated group are more explicitly controlled for.

The two additional matchings entail exactly the same procedure as the one conducted in Section 5.1. However, when firms were present in the two waves of the CIS survey, they were matched to their own past observation. These firms were observed in the same (18 Flemish and 82 German non-treated firms) or opposite (26 Flemish and 36 German firms, non-treated in t_0, but treated in t_1) treatment status in the two surveys. The same outcome and control variables are analyzed in the same hybrid matching procedure as before. Therefore, the intermediate matching results are not reported in this paper. The *t*-tests after the matching show that the selected control groups constitute a reliable match.

First, the final treatment effect estimations are presented for each matching separately (see Table 6). Estimation A is the result of the matching of treated (i) to non-treated (h) firms within CIS IV (period t_1); thus estimation A corresponds to the one presented in Section 5.1. Estimation B results from matching treated firms (i) in CIS IV (period t_1) to non-treated firms (k) in CIS III (period

t_0). Finally, estimation C indicates the difference in outcome variables between non-treated firms (h) in CIS IV and non-treated firms (j) in CIS III. The treatment effects A and B are always significant. The treatment effect over time (t_1 versus t_0) is in line with the treatment effect in the same period (t_1). The correction for different reactions to macroeconomics shocks between non-treated firms (h and j; estimation C) is never significant. The structure of the results is very similar in the Flemish and German sample.

Second, we use the differences (graphically relation B in Table 6 for the treated and relation C for the non-treated firms) in the variables as input in an OLS regression as we would do in an ordinary DiD approach, with the extra feature that we condition on the exogenous variables mentioned before.[9] The difference in each of the outcome variables over time is regressed on the difference over time in funding (FUNdif = 0 for the non-treated/non-treated matched-firms (h and j) and FUNdif = 1 for the treated/non-treated matched-firms (i and k)). As a time dimension is included in the analysis, the monetary variables (RD and lnRD) are deflated (EconStats, 2007).

As the regression is performed on matched samples, the t-statistics may be biased downwards and result in misleading conclusions (see e.g. Heckman et al., 1998). In order to obtain unbiased standard errors we employ the bootstrap methodology (see e.g. Efron and Tibshirani, 1993). We used 200 replications of the procedure to estimate the bootstrapped standard errors.

Table 7 shows that the treatment effect (FUNdif) is always significantly positive, with one exception for the R&D expenditure in the Flemish sample; this insignificance however may be due to the skewed distribution of R&D expenditure and the relatively small sample size. When the R&D intensity or the logarithmically rescaled variable is evaluated, the additionality effect is again significantly positive. The coefficients are in line with the results that only take the evolution over time of the treated firms into account (estimate B in Table 6). Taking relationship C into account results in minor corrections. As a further robustness analysis we also include the difference in the other continuous variables.[10] For the German sample we can take the EAST dummy into account as well, as this dummy was included in the hybrid matching: only companies with the same value for EAST are matched. Although these extra variables add to the explanatory power of the model, they are not significant in the regression. The positive impact of public funding remains strongly significant, even if we control for the differenced exogenous variables. The difference in outcome variables is due to the receipt of a grant. In the German sample, some differenced exogenous variables

[9] As the coefficients for relationship C are not significant in our first outcome presentation (see Table 6), it is not possible to merely subtract coefficient C from coefficient B for each outcome variable to obtain a corrected coefficient; the difference-in-differences approach allows us to bring the matching procedures B and C together.

[10] Through the triple matching procedure, we explicitly condition the selection of non-treated firms on their exogenous characteristics. This however does not mean that no differences exist in the differenced exogenous variables.

Table 7
Treatment effect estimations: OLS in differences

Variable	RDdif		RDINTdif		lnRDdif		lnRDINTdif	
Flemish sample (Number of observations: 314)								
FUNdif	0.661 (0.588)	0.571 (0.600)	5.204 (1.170)***	5.158 (1.224)***	2.574 (0.528)***	2.444 (0.525)***	2.129 (0.415)***	2.144 (0.498)***
lnEMPdif		1.505 (2.742)		−2.461 (4.163)		1.069 (1.738)		0.227 (1.760)
PATSTdif		0.461 (0.545)		0.324 (1.232)		0.786 (0.892)		0.541 (0.499)
EXQUdif		4.727 (6.644)		12.668 (15.079)		2.185 (6.076)		1.427 (5.002)
R^2	0.064	0.134	0.109	0.132	0.118	0.161	0.124	0.150
German sample (Number of observations: 968)								
FUNdiff	2.922 (1.187)**	3.529 (1.351)**	5.509 (0.598)***	4.871 (0.699)***	2.856 (0.249)***	2.644 (0.296)***	3.877 (0.316)***	3.466 (0.362)***
lnEMPdif		8.062 (5.316)		−2.886 (1.971)		0.054 (0.809)		−0.952 (0.933)
PATSTdif		0.483 (0.399)		0.389 (0.478)		0.169 (0.158)		0.134 (0.192)
EXQUdif		3.258 (3.797)		0.500 (2.117)		0.636 (1.342)		0.425 (1.778)
EAST		−1.179 (0.733)		1.256 (0.574)*		0.447 (0.442)		0.812 (0.668)
R^2	0.013	0.040	0.184	0.197	0.233	0.236	0.249	0.246

Bootstrapped standard errors (between brackets) are heteroscedastic consistent. *** (**, *): significant at 1% (5%, 10%).

Table 8
Descriptive statistics of the additional variables

Variable	Subsidized companies		Potential control group of non-subsidized companies		p-values of two-sided t-test on mean equality
	Mean	Standard deviation	Mean	Standard deviation	
Flemish sample					
PROJ_PAST5YR	1.329	3.546	0.063	0.302	$p = 0.0000$
CAPINT	0.035	0.035	0.042	0.066	$p = 0.0691$
CASHF	0.013	0.047	0.016	0.115	$p = 0.5743$
SCOMNACE	1.080	0.456	0.853	0.471	$p = 0.0000$
Number of observations	167		696		
German sample					
TECHCONSTR	0.713	0.693	0.728	0.761	$p = 0.6853$
FINCONSTR	1.781	1.108	1.365	1.155	$p = 0.0000$
SCOMNACE	1.283	0.283	1.095	0.316	$p = 0.0000$
Number of observations	488		1643		

are significant, but the main impact on outcome variables comes from the strongly significant relationship with the subsidy receipt. Even though the funding systems in Flanders and Germany are slightly different, the additionality effects have the same structure.

5.3. Robustness checks

To support the evidence that the full crowding-out hypothesis can be rejected, we provide some extra robustness checks. First, we limit the sample to R&D active companies. Next, we add variables to the analysis.

5.3.1. Only R&D active companies

Czarnitzki (2006) shows that not only the R&D expenditure but also the R&D status may change when a subsidy is granted. Small firms and firms that can offer only limited surety may experience great difficulties in raising external capital for risky projects. Consequently, only a limited budget is available for R&D activities, which may be shut down as a result. As Lerner (1999) argued, the subsidy receipt may serve as a certification of the firm's activities, which could convince potential financiers. Up until now the switch of R&D status was taken into account, as we allowed the possibility that a funded R&D active company is matched to a non-funded non-R&D active company. If we limit the sample to innovating companies only, the treatment effect may be underestimated. However, it provides a robustness check, so we conducted the analysis, selecting only R&D active companies from the CIS IV wave. For both the Flemish and German sample the treatment effect remains significantly positive, but is – as expected – somewhat lower. The samples reduce to 415 (121) matched German (Flemish) companies. The R&D

Table 9
Probit estimations and marginal effects

	Flemish sample				German sample			
	Probit estimates		Marginal effects		Probit estimates		Marginal effects	
	Coefficient	Standard error	dy/dx	Standard error	Coefficient	Standard error	dy/dx	Standard error
lnEMP	0.163	0.116	0.023*	0.013	0.000	0.057	0.018**	0.007
PATST	0.248***	0.078	0.062***	0.021	−0.001	0.002	−0.000	0.001
GROUP°	−0.066	0.154	−0.012	0.039	0.113	0.076	0.028	0.020
FOREIGN°	−0.530***	0.179	−0.098***	0.036	−0.064	0.113	−0.023	0.028
EXQU	0.686***	0.207	0.199***	0.048	1.078***	0.154	0.292***	0.040
PROJ_PAST5YR	0.856***	0.110	0.210***	0.031				
CAPINT/TECHCONSTR	−1.868	1.335	−0.440	0.323	−0.126**	0.048	−0.035**	0.013
CASHF/FINCONSTR	−0.373	0.620	−0.058	0.162	0.230***	0.031	0.062***	0.008
SCOMNACE	0.246*	0.149	0.059*	0.036	0.726***	0.143	0.191***	0.037
EAST°					0.788***	0.073	0.229***	0.023
Constant	−2.008***	0.522			−2.609***	0.276		
BR	$\chi^2(12) = 37.37$ $p = 0.0002$				$\chi^2(11) = 20.80$ $p = 0.0355$			
BR_lnEMP	$\chi^2(12) = 26.52$ $p = 0.0091$				$\chi^2(11) = 25.39$ $p = 0.0080$			
Log-likelihood	−289				−926			
Pseudo R^2	0.318				0.1927			
Number of observations	863				2131			

*** (**, *) indicate a significance level of 1% (5%, 10%). The restriction to common support is not yet being enforced here. The marginal effects on subsidies are calculated at the sample means for continuous variables and for a discrete change of dummy variables (indicated by °) from 0 to 1. The interaction terms BR_lnEMP are not taken into account in the calculation of the marginal effects. Their standard errors are obtained by the delta method.

Table 10
Outcome variables of the Flemish and German matched samples

Variable	Subsidized companies		Selected control group of non-subsidized companies		p-values of two-sided t-test on mean equality[a]
	Mean	Standard deviation	Mean	Standard deviation	
Flemish sample					
RD	1.142	2.666	0.356	0.675	$p = 0.001$
RDINT	7.080	13.637	1.913	3.779	$p = 0.000$
lnRD	−2.421	3.513	−4.406	4.113	$p = 0.000$
lnRDINT	−0.142	2.826	−1.921	3.064	$p = 0.000$
Number of observations	136		136		
German sample					
RD	4.404	16.633	3.906	17.378	$p = 0.720$
RDINT	7.191	9.677	2.524	5.197	$p = 0.000$
lnRD	−0.959	2.637	−3.178	3.493	$p = 0.000$
lnRDINT	0.394	2.887	−2.763	3.996	$p = 0.000$
Number of observations	474		474		

[a] *t*-statistics to test the mean equality between the sample of funded firms and the selected control group are based on Lechner's (2001) asymptotic approximation of the standard errors that accounts for sampling with replacement in the selected control group.

Table 11
Average treatment effects on the treated companies

	Flanders				Germany			
	Absolute		Relative (%)		Absolute		Relative (%)	
	Mean	Median	Mean	Median	Mean	Median	Mean	Median
RD (in mio EUR)	0.786	0.141	69	60	n.s.	0.361	n.s.	84
RDINT (in %)	5.167	1.069	73	72	4.667	2.949	65	91

n.s.: not significantly different.

intensity in Germany increases with 3.7% (significant at 1%) and the Flemish R&D intensity is 2.8% higher (significant at 5%), compared to an additionality effect of 5% for both countries (both significant at 1%), when all firms are kept in the analysis.

5.3.2. Additional variables

As mentioned before, a weakness of the additionality analysis presented in this paper, lies in the potential omittance of relevant variables, resulting in a violation of the conditional independence assumption. The analyses presented below include more information (PROJ_PAST5YR, CAPINT, CASHF, TECHCONSTR, FINCONSTR and SCOMNACE), which is however not available or perfectly comparable for both samples and therefore less interesting if the reader wants to compare the impact of the S&T policy in both countries. Nevertheless, the models remain comparable to a certain extent (as they reflect more or less the same information) and they provide compelling evidence, showing that the inclusion of more specific and fine-tuned information confirms the rejection of the full crowding-out hypothesis found earlier in this paper.

The computation of the additional variables results in a total sample of 4184 (1605) German (Flemish) observations on 3903 (1418) companies: the overlap between the two waves if even more limited: only 281 (187) German (Flemish) firms are observed twice. Of these firms, 14 Flemish and 46 German firms were observed in the same non-treated status and 17 Flemish and 18 German firms in the switching status (non-funded to funded) and consequently matched to their own past observation. The monetary variables (RD, lnRD, CAPINT and CASHF) are deflated in the CDiDRCS.

As the reduction of the dataset does not alter the descriptive statistics of the variables which were used in the initial analyses (see Table 2), we limit the descriptive statistics to the additional variables (see Table 8). The new probit model in Table 9 reveals the extent to which the additional variables are important factors in the selection process to receive a subsidy. Experience in applying for a subsidy clearly is a strong asset: it significantly increases the likelihood to receive a subsidy; unfortunately this variable is only available in the Flemish dataset. The financial and technological quality of the company do not seem to be of importance in Flanders; but are crucial features for German firms: financially constrained firms are more likely to receive a subsidy, while firms facing technological difficulties are less likely to be subsidized. Firms that absorb information of competitors more easily also have a significantly higher chance of receiving a subsidy. Table 10 shows the differences in the outcome variables after the matching (the *t*-tests on the other variables are not reported, as all differences were eliminated). The average treatment effects are calculated in Table 11: they remain significantly positive. Also after adding a time dimension to control for unobserved heterogeneity (Table 12), the conclusion remains stable: the hypothesis of full crowding-out can be rejected, both in the Flemish and German case.

Table 12
Treatment effect estimations: OLS in differences

Variable	RDdif		RDINTdif		lnRDdif		lnRDINTdif	
Flemish sample (Number of observations: 272)								
FUNdif	0.269 (0.571)	0.253 (0.595)	4.101 (1.413)***	3.535 (1.242)***	1.593 (0.562)**	1.547 (0.624)*	1.471 (0.475)***	1.398 (0.498)**
lnEMPdif		0.736 (1.347)		−4.815 (4.086)		0.259 (1.737)		−0.554 (1.294)
PATSTdif		0.438 (0.723)		0.683 (1.484)		0.521 (0.885)		0.396 (0.740)
GROUP		0.097 (0.406)		2.386 (1.798)		0.828 (0.808)		0.730 (0.696)
FOREIGN		0.477 (0.898)		1.977 (2.911)		−0.379 (1.486)		−0.365 (1.219)
EXQU		−0.300 (0.831)		−0.466 (3.012)		1.591 (1.315)		1.073 (1.065)
PROJ_PAST5YR		0.250 (0.551)		2.424 (1.490)		1.138 (0.636)**		1.045 (0.561)**
CAPINT		−4.869 (8.515)		−19.586 (16.690)		−6.763 (9.134)		−7.189 (7.792)
CASHF		7.704 (9.325)		−4.648 (32.289)		3.503 (11.249)		0.550 (9.739)
SCOMNACE		−0.109 (0.445)		−0.275 (1.731)		0.917 (0.731)		0.773 (0.622)
R^2	0.000	0.050	0.047	0.170	0.037	0.153	0.045	0.181
German sample (Number of observations: 948)								
FUNdif	2.009 (0.883)**	2.305 (1.143)**	5.384 (0.515)***	4.575 (0.581)***	2.571 (0.272)***	2.337 (0.312)***	3.464 (0.353)***	3.046 (0.387)***
lnEMPdif		6.759 (6.546)		−2.815 (2.021)		0.398 (0.957)		−0.612 (1.095)
PATSTdif		0.147 (0.209)		0.042 (0.033)		0.025 (0.022)		0.025 (0.022)
GROUP		1.828 (1.842)		0.588 (1.153)		0.677 (0.503)		0.855 (0.702)
FOREIGN		0.196 (2.926)		−0.770 (1.030)		−0.216 (0.594)		−0.646 (0.686)
EXQU		4.496 (5.078)		4.051 (2.098)		1.959 (1.184)		2.111 (1.569)
TECHCONSTR		−0.895 (1.137)		−0.265 (0.471)		0.030 (0.250)		0.134 (0.330)
FINCONSTR		0.573 (1.236)		0.547 (0.515)		0.289 (0.244)		0.434 (0.334)
SCOMNACE		6.014 (4.216)		4.495 (1.556)***		2.620 (0.943)		3.317 (1.140)***
EAST		−0.882 (0.749)		1.636 (0.677)**		0.466 (0.415)		0.887 (0.630)
R^2	0.007	0.088	0.155	0.199	0.196	0.230	0.203	0.241

Bootstrapped standard errors (between brackets) are heteroscedastic consistent. *** (**, *): significant at 1% (5%, 10%).

6. Conclusions

We empirically tested whether public R&D subsidies crowd out private R&D investment in Flanders and Germany, using data from the CIS III and IV waves. The main concern in evaluation analysis is to tackle the problem of selection bias. Several methods are available to solve this problem, each with specific advantages and disadvantages. First, hybrid nearest neighbor matching was employed in the CIS IV cross-sectional sample. The sample contains information on the funding status and other covariates in the period 2002–2004. For both samples the crowding-out hypothesis was rejected: on average, the R&D intensity of German (Flemish) funded companies is 76–100% (64–91%) higher than the R&D intensity of non-funded companies. The disadvantage of the matching estimator is that it does not control for unobserved heterogeneity. Therefore, we applied a combination of the matching procedure and the difference-in-differences method, i.e. conditional difference-in-differences, using the two cross-sections CIS III and IV. This estimator allows correcting for both observed and unobserved heterogeneity. Also in this case, the crowding-out hypothesis can clearly be rejected; funded firms are significantly more R&D active than non-funded firms. Further robustness checks, like limiting the sample to R&D active companies only and taking additional, more fine-tuned variables into account, lead to the same results. The conclusions are in line with results from earlier studies on additionality in Flanders and Germany and also other European countries.

Only the funding status of firms is analyzed. Therefore it is not possible to indicate how much R&D expenditure is leveraged with 1 EUR extra funding. This has been tested for a cross-section of Flemish data. It would be interesting to employ continuous treatment analysis in a time-series framework for both countries and in this way test for heterogeneous treatment effects of subsidies. Another appealing research question is the additionality effect on the output side. Input additionality is not necessarily translated into innovative output and economic welfare. Very recently, studies have been conducted on output additionality, measured in terms of patents, in German firms (Czarnitzki and Hussinger, 2004 as well as Czarnitzki and Licht, 2006). In addition to these studies, it would be interesting to look at other innovation indicators on the output side of the innovation process, such as the introduction of new products or processes. A first study using a dummy variable on the introduction of an innovation into the market has been conducted by Hujer and Radić (2005). However, long time-series data would give more insight and would allow testing different lag specifications between the moment of market introduction of new products or the implementation of new processes and the time period in which the corresponding R&D projects were actually conducted.

Acknowledgements

This paper was written during a research stay of Kris Aerts at the ZEW. She would like to thank the ZEW for its hospitality and financial support. Moreover, the authors are indebted to Birgit Aschhoff, Michel Callon, Dirk Czarnitzki and two anonymous referees for their valuable and highly appreciated comments. This paper represents the authors' personal opinion and does not necessarily reflect the views of the Deutsche Bundesbank or its staff.

References

Acs, Z.J., Audretsch, D.B., 1987. Innovation, Market Structure, and Firm Size. The Review of Economics and Statistics 69 (4), 567–574.
Aerts, K., Czarnitzki, D., 2004. Using innovation survey data to evaluate R&D policy: the case of Belgium, ZEW Discussion Paper 04-55, Mannheim. Also appeared as Research Report OR 0439, K.U. Leuven, Dept. of Applied Economics.
Aerts, K., Czarnitzki, D., 2006. The Impact of Public R&D Funding in Flanders, 54. IWT M&A study.
Aerts, K., Czarnitzki, D., Fier, A., 2007. Capítulo 3: Evaluación econométrica de las políticas públicas de I + D: situación actual. In: Heijs, J., Buesa, M. (Eds.), La cooperación en innovación en España y el papel de las ayudas públicas. Instituto de Estudios Fiscales, Madrid, pp. 79–104.
Almus, M., Czarnitzki, D., 2003. The effects of public R&D subsidies on firms' innovation activities: the case of Eastern Germany. Journal of Business and Economic Statistics 21 (2), 226–236.
Arrow, K.J., 1962. Economic welfare and the allocations of resources of invention. In: Nelson, R.R. (Ed.), The Rate and Direction of Inventive Activity: Economic and Social Factors. Princeton, pp. 361–392.
Aschhoff, B., Doherr, T., Ebersberger, B., Peters, B., Rammer, C., Schmidt, T., 2006. Innovation in Germany—Results of the German Innovation Survey 2005, Mannheim.
Blundell, R., Costa Dias, M., 2000. Evaluation methods for non-experimental data. Fiscal Studies 21 (4), 427–468.
Blundell, R., Costa Dias, M., 2002. Alternative approaches to evaluation in empirical microeconomics. Portuguese Economic Journal 1, 1–38.
Busom, I., 2000. An empirical evaluation of the effects of R&D subsidies. Economics of Innovation and New Technology 9 (2), 111–148.
Commission of the European Communities, 2000. Innovatie in een kenniseconomie, Brussel.
Czarnitzki, D., 2001. Die Auswirkungen der Forschungs- und Technologiepolitik auf die Innovationsaktivitäten ostdeutscher Unternehmen. Schmollers Jahrbuch - Zeitschrift für Wirtschafts- und Sozialwissenschaften 121 (4), 1–22.
Czarnitzki, D., 2006. Research and development in small and medium-sized enterprises: the role of financial constraints and public funding. Scottish Journal of Political Economy 3 (53), 335–357.
Czarnitzki, D., Fier, A., 2002. Do Innovation subsidies crowd out private investment? evidence from the German service sector. Konjunkturpolitik—Applied Economics Quarterly 48 (1), 1–25.
Czarnitzki, D., Hussinger, K., 2004. The link between R&D subsidies, R&D input and technological performance, ZEW Discussion Paper 04–56, Mannheim.
Czarnitzki, D., Licht, G., 2006. Additionality of public R&D grants in a transition economy: the case of Eastern Germany. Economics of Transition 14 (1), 101–131.
David, P.A., Hall, B.H., Toole, A.A., 2000. Is public R&D a complement or substitute for private R&D? A review of the econometric evidence. Research Policy 29 (4–5), 497–529.
Duguet, E., 2004. Are R&D subsidies a substitute or a complement to privately funded R&D? Evidence from France using propensity score methods for non experimental data. Revue d'Economie Politique 114 (2), 263–292.
EconStats, 2007. GDP Deflator.
Efron, B., Tibshirani, R.J., 1993. An introduction to the bootstrap, New York.
Eurostat, 2004. Innovation in Europe—Results for the EU, Iceland and Norway, Luxembourg.
Fier, A., 2002. Staatliche Förderung industrieller Forschung in Deutschland, ZEW Wirtschaftsanalysen, Bd. 62, Baden-Baden.
González, X., Pazó, C., 2006. Do Public Subsidies Stimulate Private R&D Spending?, Documentos de Traballo 0601. Universidade de Vigo, Departamento de Economía Aplicada.
González, X., Jaumandreu, J., Pazó, C., 2005. Barriers to innovation and subsidy effectiveness. RAND Journal of Economics 36 (4), 930–950.
Görg, H., Strobl, E., 2007. The effect of R&D subsidies on private R&D. Economica 74 (294), 215–234.
Griliches, Z., Mairesse, J., 1984. Productivity and R&D at the firm level. In: Griliches, Z. (Ed.), R&D. Patents and Productivity, Chicago, pp. 339–374.
Griliches, Z., 1986. Productivity, R&D, and basic research at the firm level in the 1970's. American Economic Review 76 (1), 141–155.

Griliches, Z., 1990. Patent statistics as economic indicators: a survey. Journal of Economic Literature 2 (4), 1661–1797.

Heckman, J.J., Ichimura, H., Todd, P., 1998. Matching as an econometric evaluation estimator. Review of Economic Studies 65 (2), 261–294.

Heckman, J.J., Lalonde, R.J., Smith, J.A., 1999. The economics and econometrics of active labor market programs, in: Ashenfelter, A., Card, D., (Eds.), Handbook of Labor Economics 3, Amsterdam, 1866–2097.

Heijs, J., Herrera, L., 2004. The distribution of R&D subsidies and its effect on the final outcome of innovation policy, Working paper Instituto de Análisis Industrial y Financiero 46, Madrid.

Hujer, R., Radić, D., 2005. Evaluating the impacts of subsidies on innovation activities in Germany, ZEW Discussion Paper 05-43, Mannheim.

Hussinger, K., 2008. R&D and subsidies at the firm level: an application of parametric and semi-parametric two-step selection models, Journal of Applied Econometrics, forthcoming.

Jaffe, A.B., 1986. Technological opportunity and spillovers of R&D: evidence from firm's patent, profits, and market value. American Economic Review 76 (5), 984–1001.

Kaiser, U., 2004. Private R&D and public R&D subsidies: Microeconometric evidence from Denmark, CEBR Discussion Paper 2004-19, Denmark.

Lach, S., 2002. Do R&D subsidies stimulate or displace private R&D? Evidence from Israel. Journal of Industrial Economics 50 (4), 369–390.

Lechner, M., 1998. Training the East German labour force: microeconometric evaluations of continuous vocational training after unification, Heidelberg.

Lechner, M., 2001. Identification and estimation of causal effects of multiple treatments under the conditional independence assumption. In: Lechner, M., Pfeiffer, F. (Eds.), Econometric Evaluation of Active Labor Market Policies, Heidelberg, 43–58.

Lerner, J., 1999. The government as venture capitalist: the long-run impact of the SBIR program. Journal of Business 72 (3), 285–318.

Lööf, H., Heshmati, A., 2005. The impact of public funding on private R&D investment. New evidence from a firm level innovation study, CESIS Working Paper 06, Sweden.

OECD, 1993. Proposed Standard Practice for Surveys of Research and Experimental Development - Frascati Manual, Paris.

OECD/Eurostat, 1997. Proposed Guidelines for Collecting and Interpreting Technological Innovation Data - Oslo Manual, Paris.

OECD, 2006a. Government R&D Funding and Company Behaviour: Measuring Behavioural Additionality, Paris.

OECD, 2006b. Main Science and Technology Indicators, Paris.

Rosenbaum, P.R., Rubin, D.B., 1983. The central role of the propensity score in observational studies for causal effects. Biometrika 70 (1), 41–55.

Roy, A.D., 1951. Some Thoughts on the distribution of earnings. Oxford Economic Papers 3 (2), 135–146.

Rubin, D.B., 1977. Assignment to treatment group on the basis of covariate. Journal of Educational Statistics 2, 1–26.

Rubin, D.B., 1990. Formal mode of statistical inference for causal effects. Journal of Statistical Planning and Inference 25 (3), 279–292.

Samuelson, P.A., 1954. The pure theory of public expenditure. Review of Economics and Statistics 36 (4), 387–389.

Sofka, W., Schmidt, T., 2004. I Like the Way you Move - An Empirical Investigation into the Mechanisms Behind First Mover and Follower Strategies, ZEW Discussion Paper No. 04-87, Mannheim.

Suetens, S., 2002. R&D subsidies and production effects of R&D personnel: evidence from the Flemish region, CESIT Discussion Paper 2002/03, Antwerp.

Van Pottelsberghe, B., Nysten, S., Megally, E., 2003. Evaluation of Current Fiscal Incentives for Business R&D in Belgium. Solvay Business School and Service Public Federal de Programmation Politique Scientifique, Brussels.

Veugelers, R., Cassiman, B., 1999. Make or buy in innovation strategies: evidence from Belgian manufacturing firms. Research Policy 28 (1), 63–80.

Wallsten, S.J., 2000. The effects of government-industry R&D programs on private R&D: the case of the Small Business Innovation Research Program. RAND Journal of Economics 31 (1), 82–100.

Wooldridge, J.M., 2002. Econometric Analysis of Cross Section and Panel Data. Massachusetts Institute of Technology Press, Cambridge/London.